



Moving On Up? A Virtual School, Student Mobility, and Achievement

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Virtual charter schools provide full-time, tuition-free K-12 education through internet-based instruction. Although virtual schools offer a personalized, content-appropriate experience, most research suggests these schools are negatively associated with achievement. Few studies account for differential rates of student mobility, which may produce biased estimates if mobility is jointly associated with virtual school enrollment and subsequent test scores. We account for student mobility in an evaluation of a single, large, anonymous virtual charter school. We estimate treatment effects of the virtual school on student achievement using a hybrid of exact and nearest-neighbor propensity score matching. Relative to their matched peers, we estimate that virtual students produce similar ELA scores and significantly worse math scores after one year. Among a limited sample of students observed for four years, we estimate that virtual students ultimately produce higher ELA scores and similar math scores relative to matched peers. We argue these findings are more reliable indicators of the independent effect of virtual schooling on student achievement because the match on student mobility is a proxy for otherwise unobservable negative selection factors.

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Abstract

Virtual charter schools provide full-time, tuition-free K-12 education through internet-based instruction. Although virtual schools offer a personalized, content-appropriate experience, most research suggests these schools are negatively associated with achievement. Few studies account for differential rates of student mobility, which may produce biased estimates if mobility is jointly associated with virtual school enrollment and subsequent test scores. We account for student mobility in an evaluation of a single, large, anonymous virtual charter school. We estimate treatment effects of the virtual school on student achievement using a hybrid of exact and nearest-neighbor propensity score matching. Relative to their matched peers, we estimate that virtual students produce similar ELA scores and significantly worse math scores after one year. Among a limited sample of students observed for four years, we estimate that virtual students ultimately produce higher ELA scores and similar math scores relative to matched peers. We argue these findings are more reliable indicators of the independent effect of virtual schooling on student achievement because the match on student mobility is a proxy for otherwise unobservable negative selection factors.

Keywords: virtual schools, charter schools, student mobility, matching, quasi-experimental design, on-line learning

Background on Virtual Schooling

Virtual schools, also referred to as cyber schools or online schools, provide full-time, tuition-free K-12 education to nearly 300,000 students in 35 states (Molner et al., 2019). Although virtual students usually receive education from their home residence, virtual schooling is distinct from homeschooling. The defining feature of virtual schooling is the web-based delivery mechanism. Virtual schools often provide computers and software, and students are connected to instructors through the internet, video-conference, email, and telephone (Gill et al., 2015). Lessons may be synchronous or asynchronous, depending on the school and course, allowing students to interact with teachers and peers in real-time or progress independently. As of 2018, one-fifth of virtual students were enrolled in district-managed virtual schools (Molner et al., 2019). One of the first district-managed virtual schools was established by Florida’s legislature in 1997.¹

Virtual charter schools, one form of virtual schooling, are state-funded, state-regulated public schools that educate students through internet-based communication. As of January 2020, virtual charter schools were authorized in 27 states.² Virtual charters are required to administer annual state assessments like brick-and-mortar charter schools.

Pazhouh and colleagues (2015) underscore the heterogeneous nature of virtual schools in lesson delivery. Students are required to be online at different times, experience a mixture of online and in-person discussions, and have varying amounts of teacher interaction. Gill and colleagues (2018) document that Black students and students with disabilities enroll in virtual schools at similar levels to those found in other public schools, although Hispanic students and non-native English speakers are somewhat underrepresented. Moreover, Beck and colleagues

¹ Florida Virtual School. “About Florida Virtual School.” Retrieved from: <https://www.flvs.net/about/flvs>

² Education Commission of the States. (2020). “Charter Schools: Does state law explicitly allow virtual charter schools?” Retrieved from: https://www.crpe.org/sites/default/files/crpe-policy-framework-online-charter-schools-final_0.pdf

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(2014) present evidence that virtual schools serve disproportionately high numbers of students with emotional or academic problems relative to traditional public schools.

Advocates argue that virtual schools reimagine education through an innovative approach, personalized to the unique needs of each student. Virtual schooling has potential to offer instruction for children in remote areas who lack access to certain coursework (Heppen et al., 2011). In addition, virtual schools can hire teachers from anywhere in each state, accommodate teachers who prefer non-traditional work schedules, and employ large teacher-student ratios without sacrificing personalized attention for students (Pazhouh et al., 2015). Virtual schools are also attractive for students with rare illnesses or unusual travel schedules.

Parents may pursue non-traditional educational models for various reasons, including academic quality, civic or religious values, concerns about safety, curriculum, class size, extracurriculars, school facilities, and longer school hours, among others. School choice theory suggests student achievement will improve when parents exercise increased autonomy over the education of their children (Chubb & Moe, 1990).

Nonetheless, virtual charter schools are controversial. Critics point to virtual schools' poor track record of increasing achievement relative to traditional public schools. Researchers question whether "the poor performance of virtual charter schools reflects a disadvantage inherent to online instruction, the unique dysfunction of this particular sector of schools, or some combination of the two factors."³ In several states, a combination of unions,⁴ school board

³ Fitzpatrick, B. R., Berends, M., Ferrare, J.J., & Waddington, J. (2020). "Virtual charter schools and online learning during COVID-19." Retrieved from Brookings Institution: <https://www.brookings.edu/blog/brown-center-chalkboard/2020/06/02/virtual-charter-schools-and-online-learning-during-covid-19/>

⁴ "Oregon's Coronavirus education lockdown." (2020). Retrieved from *Wall Street Journal*: <https://www.wsj.com/articles/oregons-coronavirus-education-lockdown-11585697080>

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organizations,⁵ and district leaders⁶ have called for virtual school moratoriums, enrollment limits, and spending cuts. But it is not just opponents of school choice who oppose virtual schooling. In 2016, three national organizations that support brick-and-mortar charter schools issued a report highlighting the need to improve the quality of virtual charter schools.⁷

Virtual Schooling Literature Suggests Negative Association with Achievement

There have been few rigorous evaluations comparing outcomes of virtual students to their traditional brick-and-mortar public school peers (Barbour & Reeves, 2009; U.S. Department of Education, 2010). Even fewer studies account for the type of students who select into virtual schooling. Virtual students may be, on average, negatively selected based on prior test scores, disability status, and poor experience in a previous school. A virtual student also may be positively selected if he or she is unchallenged academically in traditional schools.

Existing evaluations conclude that virtual schooling has significant and large negative effects on student achievement, especially in math. None of the studies of virtual schooling have been experimental, since online instruction can be provided to most students who desire it. It is not clear if the quasi-experimental studies to date have adequately controlled for observable and unobservable factors that might bias estimates of virtual schooling effects.

A research team with the Center for Research on Education Outcomes at Stanford University systematically explored the effects of virtual charter schools on student achievement

⁵ “PSBA supports governor taking steps to address charter funding issues.” (2019). Retrieved from Pennsylvania School Boards Association: <https://www.psba.org/2019/08/psba-supports-governor-taking-steps-to-address-charter-funding-issues/>

⁶ Mezzacappa, D. (2020). “District leaders call for moratorium on new charters until law is changed.” Retrieved from *Philadelphia Public School Notebook*: <https://thenotebook.org/articles/2020/01/27/district-leaders-call-for-moratorium-on-new-charters-until-law-is-changed/>

⁷ “A call to action to improve the quality of full-time virtual charter public schools.” (2016). Retrieved from National Association of Public Charter Schools: <https://www.publiccharters.org/sites/default/files/migrated/wp-content/uploads/2016/06/Virtuals-FINAL-06202016-1.pdf>

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in multiple states (Woodworth et al., 2015). Researchers collaborated with 18 state departments of education to obtain data on virtual charter and traditional public school students. The evaluation employed a virtual control record (VCR) matching method to compare students in virtual schools to similar students in traditional public schools. VCR created an artificial comparison group based on the average characteristics of multiple students. For each observed virtual charter student, a match was generated by drawing on the records of public school students with identical traits and similar prior achievement. Although the researchers provided descriptive statistics on student mobility, they did not include mobility as a control variable in their statistical models. The study found large negative results for virtual charter students compared to peers in traditional public schools. Virtual charter students produced 25 percent of a standard deviation lower gains in math and 10 percent of a standard deviation lower gains in English language arts (ELA).

Other quasi-experimental research also found negative associations between virtual charter attendance and student achievement. For example, Fitzpatrick and colleagues (2020) used longitudinal student records in Indiana to compare students who switch from traditional public schools to virtual charters against matched peers with similar observable characteristics. The authors found that virtual students perform one-third of a standard deviation worse in ELA and one-half of a standard deviation worse in math compared to their peers. Ahn and McEachin (2017) found virtual students in Ohio perform significantly worse than comparable students in traditional schools in both math (roughly 40 percent of a standard deviation) and ELA (roughly 20 percent of a standard deviation). Additionally, Bueno (2020) used individual fixed effects to analyze the effects of virtual schooling among students who switch between virtual and traditional public schools in Georgia. Here, virtual students demonstrated achievement losses

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ranging from 10 percent to 40 percent of a standard deviation in multiple subjects. An evaluation of a virtual charter school in a southern state, using a matching design, found that participating students experienced statistically significant negative achievement effects in ELA and math during their first three years of virtual schooling (Lueken et al., 2015). However, students who remained in the virtual school for longer than three years recovered and eventually produced greater gains in years four and five, compared to the matched comparison group.

Our interest is in public charter schools that deliver instruction through a virtual modality. The intervention is a composite of charter schooling and virtual schooling. A separate research literature exists on virtual schooling delivered by traditional public schools (e.g., Evergreen Education Group, 2017; Schwerdt & Chingos, 2015). As has been made clear during the Covid crisis, charter schools and traditional public schools tend to deliver virtual schooling differently, whether under planned or unplanned circumstances (Kingsbury, 2020; Vanourek, 2020). Students also vary in the extent to which they use computer technology at home to complete schoolwork, with some studies indicating more home computer use is associated with lower student achievement (Agasisti, Gil-Izquierdo & Han, 2020; Fairlie, Beltran & Das, 2010). The general research literature on education technology is vast and variegated. Thus, we focus on the narrower research literature specifically on virtual charter schooling.

Student Mobility: A Strong Predictor of Low Achievement

A student may be considered mobile if he or she transfers from one school to another for reasons other than a grade promotion. In our study, student mobility is measured by counting the number of transfers that occur during a school year. School transfers during the academic year are likely to harm student learning since they interrupt the sequencing of concepts and skills. Student

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mobility, also referred to as churn or transience, can occur for many reasons. Families may seek academic programming at a different school, a parent may assume a new job in a new location, or the student may suffer from bullying or dissatisfaction in the previous school (Sparks, 2016).

A meta-analysis by Mehana and Reynolds (2004) found student mobility is a strong, negative predictor of achievement. Similarly, Hanushek & colleagues (2004) estimated that student learning losses, particularly those experienced in the first year, had spillover effects on other students. Kerbow (1996) found that high rates of student mobility had large negative implications for schools in addition to the students themselves. South and colleagues (2007) used the National Longitudinal Study of Adolescent Health to estimate that mobile students were twice as likely as non-mobile peers to drop out of school, although mobile students differed on observable variables that may have influenced both mobility and persistence.

Virtual students are highly mobile. Gill and colleagues (2018) find that the average student in a virtual school tends to enroll for only two years. As a result, some researchers studying virtual schools argue that student mobility should be included in statistical models that compare achievement of virtual students to traditional public school students (Gatti, 2018).

Data

We study a full-time virtual charter school. Any K-12 student residing within the anonymous state is eligible to attend, tuition-free. All students are required to take standardized assessments in the grades mandated by the state's accountability system. We use statewide student-level achievement and demographic data from the 2014-15 through 2017-18 school years. The outcomes of interest are ELA and math scores, standardized by year and grade. The data include indicators for gender and race, as well as free- or reduced-price lunch (FRPL) eligibility, English as a Second Language (ESL) status, and special education (SPED) status.

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Treatment is defined as being enrolled in the virtual school of interest at any time during the outcome year. The comparison condition is defined as not being enrolled in the virtual school of interest during the outcome year. We observe if a student transferred to a different school during the school year but not specifically when the transfer occurred. Thus, we code any student exposed to the virtual charter school during the outcome year as “treated.”

The dataset contains 12,498 unique students in the virtual school and 1,061,165 other public school students with whom the virtual students may be compared. We limit the sample to grades 3 through 8, reducing the analytic sample to 6,054 virtual students and 574,633 potential comparison students since these are the grade levels assessed by the state. Then, because students must have baseline and outcome test scores to be included in our analysis, the sample is further reduced to slightly more than 3,500 virtual students and slightly less than 400,000 comparison students (Table 1). Student counts in ELA are slightly different from student counts in math because of variation in missing test score data, explained in more detail below.

Table 1: Unique Students Who Meet Criteria for Inclusion in Analytic Samples

	<u>ELA</u>		<u>Math</u>	
	Virtual	Comparison	Virtual	Comparison
Total Unique Students	12,498	1,061,165	12,498	1,061,165
<i>% of total unique students</i>	100%	100%	100%	100%
In Grades 3-8	6,054	574,633	6,054	574,633
<i>% of total unique students</i>	48%	54%	48%	54%
With baseline test scores	3,888	406,177	3,890	407,106
<i>% of total unique students</i>	31%	38%	31%	38%
With outcome test scores	3,505	393,632	3,509	393,824
<i>% of total unique students</i>	28%	37%	28%	37%

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To quantify mobility, we create a variable identifying each student's number of mid-year school switches during the outcome year. For example, a student who remains in the same school for an entire school year is coded as "0" mid-year switches, while a student who changes schools twice during one school year is coded with "2." Less disruptive transfers between school years—over the summer, for example—are not captured by this variable.

Table 2 presents baseline descriptive statistics for our analytic sample. Students in the virtual school have statistically significant and practically meaningful observable differences from other public school students. Virtual students demonstrate higher lagged ELA scores and lower lagged math scores than their peers. Unsurprisingly, the average virtual student has more mid-year school transfers than his or her peers. Virtual students are substantially more likely to be white and less likely to be Black. Students in the virtual school are less likely to be identified with ESL, SPED, or FRPL status. Since the virtual school does not operate a traditional school lunch program, it is possible some low-income families did not complete the necessary paperwork for their child to be deemed eligible for the program. However, FRPL eligibility can be the basis for participating in other programs, such as receiving free education technology. It is therefore unclear whether parents of students in the virtual school are less likely to submit FRPL paperwork. Absent clear evidence to the contrary, we assume that FRPL rates are not systematically biased across the treatment and comparison conditions, though we acknowledge such bias is possible.

Table 2: Descriptive Baseline Statistics (2015-16)

	Virtual	Comparison	Difference	
Lagged ELA (normed)	0.09	0.00	0.09	***
Lagged Math (normed)	-0.14	0.00	-0.14	***
Mid-year School Switches	0.35	0.06	0.29	***
Female	0.52	0.49	0.03	**
FRPL	0.57	0.60	-0.03	**
ESL	0.01	0.05	-0.04	***
Special Education	0.12	0.12	0.00	
White	0.74	0.52	0.22	***
Black	0.12	0.34	-0.22	***
Hispanic	0.06	0.08	-0.02	***
Asian	0.01	0.02	-0.01	
	n= 1,532	n= 267,658		

Notes: The number of virtual students, 1,532, is less than the number of virtual students in Table 3A and 3B because Table 3A and 3B include virtual students from multiple outcome years, not just 2015-16. The 2015-16 school year is the first year for which outcome data can be observed, because 2014-15 serves as the baseline measure of the outcome variable.

*** p < 0.01, ** p < 0.05, * p < 0.1

Missing Data

In administrative records with over one million students spanning multiple years, missing data are to be expected. Fortunately, missing data are relatively rare among both dependent and independent variables. Less than five percent of ELA or math test scores in the entire sample are missing. Conditional on observable characteristics, virtual students are six percentage points more likely than potential comparison group students to report missing achievement data, in both

subjects. Given the overall low rate of missing outcome data, this differential attrition is consistent with the quality standards of the What Works Clearinghouse.⁸

An even smaller proportion of independent variables are missing data. Less than one half of one percent of race, gender, FRPL, and ESL indicators are missing. We used multiple imputation to address these missing cells within our matrix of independent variables. The results we present in the following pages include imputed values for the independent variables. All points estimates are robust to the sample without imputed data, which is plausible given the overall low rates of missingness.

Empirical Strategy

The ideal research design to evaluate the virtual school would be an experiment with random assignment. Unlike brick-and-mortar charter schools constrained by limits on physical space, virtual schools typically can accommodate all students who seek to enroll, so random lotteries are less likely to occur. The anonymous school in our analysis does not utilize lotteries to determine enrollment.

Given that random assignment is not feasible, a credible analysis of student achievement hinges on the strategy for reducing selection bias. As is evident from the descriptive statistics (Table 2), the average virtual student differs from the average non-virtual student in observable ways. Accordingly, it is likely virtual students and their peers differ on unobservable characteristics influencing both selection into virtual schooling and future achievement.

To reduce selection bias, we use a hybrid of exact and nearest-neighbor propensity score matching to match students by grade-year cohort, prior achievement, mobility, and other

⁸ U.S. Department of Education, Institute for Education Sciences, What Works Clearinghouse, Standards Handbook, Version 4.1, pg. 12.

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demographic characteristics (Rosenbaum & Rubin, 1983). These variables control for observable factors that might otherwise confound estimation of the effects of the virtual school and serve as proxies for unobservable differences between the average treated and comparison student. Our matching protocol generates a comparison group equal to the number of virtual students in the analytic sample. For example, when estimating one-year ELA outcomes, 3,412 virtual students are matched to 3,412 comparison students, producing an analytic sample of 6,824. One-to-one matching was used because the resulting analytic sample is sufficiently sized to generate a well-powered analysis. Matching rarely eliminates omitted variables bias, but it can reduce such bias to trivial levels (Bifulco, 2012).

Our analysis includes five main components. The first is an evaluation of the virtual school, comparing one-year outcomes of virtual students to a matched comparison group in other public schools. We hypothesize that controlling for student mobility will produce more accurate and reliable estimates of the effect of virtual schooling than has been estimated in other studies. Second, we explore heterogeneous one-year effects among students based on gender, race, FRPL status, and school level. Third, we compare one-year outcomes from a restricted sample of students who do not transfer schools in the outcome year.

Fourth, we investigate the role of student mobility by omitting the mobility variable from our matching protocol and comparing these estimates to those generated from the protocol that matched on mobility. We hypothesize that failing to account for student mobility will downwardly bias the estimate of virtual charter schooling. Our assumption, grounded in theory and research on school mobility, is that students with greater numbers of school transfers will be negatively selective, on average, compared to other students.

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In the fifth and final aspect of our analysis, we restrict the analytic sample to include only those students observed for all four years. Limiting the sample this way produces an inherently less-mobile, nearly immobile, student population. We generate a matched comparison group for the virtual students enrolled in their school for all four years. Test scores from 2014-15 are used as the baseline measure of the outcome variable, and the following years provide one, two, and three-year outcomes, respectively. We hypothesize that virtual students who are observed in the same school for multiple years will demonstrate greater achievement gains than similar peers by the end of the third year, relative to the difference between virtual and comparison students' one-year outcomes.

Generating Comparison Group through Matching

We use logit models to predict the likelihood that a student enters the virtual charter school:

$$Pr(D_{it}) = \delta_0 + \delta_1 A_{it-1} + \delta_2 M_{it} + \mathbf{X}_{it} \delta_3 + \varepsilon_{it}$$

Our models compare students within cohort cells, ensuring that they are matched to peers in the same grade and year. In each model, the probability of attending the virtual school (D) is a function of prior year test scores in ELA and math (A_{it-1}), an ordinal variable measuring student mobility (M), and student-level covariates (\mathbf{X}) including gender, race, FRPL status, ESL status, and SPED status. We exact-match on a band of baseline proficiency, gender, race, and mobility to limit observable differences on these crucial factors.

To limit differences in baseline achievement, virtual students are matched with non-virtual students who scored within a 5-percentage point band on the prior year's state assessment in the subject of the relevant dependent variable. Students are within-band matched on lagged ELA scores and lagged math scores separately. Thus, different comparison groups are

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constructed for math and ELA, respectively. Separate comparison groups are necessary because there are not enough comparison students who share similar lagged test scores in both subjects with virtual students. In our preferred specification, we control for prior year performance in both subjects.

After limiting the pool of potential comparison students to those with the same cohort, baseline proficiency, gender, race, and mobility,⁹ students are matched with their nearest neighbor based on propensity scores. We use a caliper of 0.10 and match without replacement.¹⁰ The protocol generates one comparison student for each virtual student in the final analytic sample. For each student pair, the outcome year is the second year that a student appears in the data, with the first appearance in the data serving as the baseline year.

Once the comparison group is identified, we estimate the effects of the virtual school on student achievement in a multiple regression framework, using the model:

$$Achievement_{it} = \alpha + \beta_1 D_{it} + \beta_2 A_{it-1} + \beta_3 M_{it} + \mathbf{X}_{it} \beta_4 + \epsilon_{it}$$

Standardized test scores in ELA or math ($Achievement_{it}$) for student i in year t are a function of an indicator variable for virtual school treatment (D); prior year achievement in both subjects (A_{it-1}); a continuous variable (M) indicating the number of mid-year school switches; and a vector (\mathbf{X}) of student-level covariates, including gender, race, FRPL, ESL, and special education status; and an error term (ϵ). The coefficient of interest is β_1 .

⁹ Students with two or more mid-year transfers are matched to other students with two or more transfers, but the exact number of transfers may differ. In other words, a virtual student with three transfers may be matched to a student with two transfers.

¹⁰ Ideally, we would identify a comparison student who is identical on observable characteristics for each virtual student in the analytic sample. In practice, there is a trade-off between match quality and the number of virtual students included in the analysis. Virtual students are dropped on the rare occasion when no suitable comparison students exist who meet the exact-matching and propensity score criteria. In the pages that follow, we report the percentage of virtual students who dropped from each matching protocol.

Results

Overview

Results are presented in five sections. First, we report that virtual students produce similar one-year outcomes in ELA and moderate-to-large negative one-year results in math relative to their matched peers. Second, we find the estimated effect of virtual charter schooling is more positive for males than females and more positive for middle school students (grades 6-8) than elementary school students (grades 3-5). Third, because the treatment group includes mobile students who were exposed to virtual and non-virtual schooling in the outcome year, we also present results from a restricted sample of students who do not transfer schools during the outcome year. We find similar point estimates for ELA and slightly more negative results in math compared to the sample that includes students who transfer during the outcome year. Fourth, we report that failing to account for mobility results in marginally more negative results for virtual students, relative to the findings in the first section. Fifth, we analyze medium-term outcomes by restricting our treated sample to students enrolled in the virtual school of interest for all four years.¹¹ By the third outcome year, we report that students in the virtual school demonstrate moderate-to-large advantages in ELA relative to their matched peers. In math, we report negative point estimates for virtual charter schooling that fall short of statistical significance by the third outcome year.

¹¹ All else equal, longer-term outcomes are more salient than single year outcomes. In this analysis, shorter-term outcomes are the focus because we only observe four years of data. Further, we only evaluate a single school which limits power to detect effects among the relatively few students observed for multiple years.

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One-Year Outcomes, Controlling for Mobility

Table 3A presents observable differences between the sample of virtual students and their matched peers used to estimate ELA outcomes.¹² The sample size is so large, at nearly 7,000 students, that even small differences between the student groups will be statistically significant. However, many of the statistically significant differences that existed between the groups prior to matching (Table 2) are eliminated after matching. Virtual students remain less likely to be identified as FRPL and ESL and are marginally more likely to have SPED status.

Note that in Table 3A, the ELA outcome sample, virtual students have similar prior ELA scores but worse prior year math scores. As discussed above, we prioritize baseline equivalence in the subject of the relevant dependent variable. For comparison, the observable differences in Table 3B, the math outcome sample, are similar to those found in Table 3A with the exception of lagged test scores. In Table 3B, the groups are indistinguishable on prior year math performance, but the virtual students are positively selected with respect to baseline ELA scores.¹³ The virtual and comparison students have similar rates of mobility in Tables 3A and 3B.

¹² Fewer than three percent of virtual students from the final analytic sample are dropped from this matching protocol.

¹³ Fewer than two percent of virtual students from the final analytic sample are dropped from this matching protocol.

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Table 3A: Observable Differences After Matching (ELA Outcome)

	Virtual	Comparison	Difference	
Lagged ELA (normed)	0.07	0.07	0.00	
Lagged Math (normed)	-0.09	0.01	-0.10	***
Mid-year School Switches	0.51	0.51	0.00	
Female	0.53	0.53	0.00	
FRPL	0.58	0.65	-0.08	***
ESL	0.01	0.04	-0.03	***
Special Education	0.09	0.08	0.01	*
White	0.73	0.73	0.00	
Black	0.15	0.15	0.00	
Hispanic	0.06	0.06	0.00	
Asian	0.01	0.01	0.00	
	n= 3,412	n= 3,412		

Notes: See appendix for student counts by grade and year.

*** p < 0.01, ** p < 0.05, * p < 0.1

Table 3B: Observable Differences After Matching (Math Outcome)

	Virtual	Comparison	Difference	
Lag ELA (normed)	0.07	-0.06	0.13	***
Lag Math (normed)	-0.09	-0.09	0.00	
Mid-year School Switches	0.51	0.51	0.00	
Female	0.53	0.53	0.00	
FRPL	0.58	0.67	-0.08	***
ESL	0.01	0.04	-0.03	***
Special Education	0.08	0.08	0.00	
White	0.73	0.73	0.00	
Black	0.15	0.15	0.00	
Hispanic	0.06	0.06	0.00	
Asian	0.01	0.01	0.00	
	n= 3,390	n= 3,390		

Notes: See appendix for student counts by grade and year. The number of virtual students is smaller for math outcomes than ELA outcomes because there are more missing outcome data for math than ELA.

*** p < 0.01, ** p < 0.05, * p < 0.1

Table 4 compares ELA and math achievement of virtual students after one year against their matched comparison group. In the preferred specification, we control for all observable characteristics, including prior year test scores. Notably, the estimated effects on ELA are indistinguishable between the groups, an unusual finding relative to other literature on virtual schooling. In math, virtual students’ achievement lags their matched peers by 18 percent of a standard deviation, a finding supported by other research. These point estimates are robust to a simple specification that only includes an indicator variable for virtual charter schooling.

Table 4: Virtual School, One-Year Outcomes with Mobility

	<u>ELA</u>		<u>Math</u>	
	<u>Simple</u>	<u>Preferred</u>	<u>Simple</u>	<u>Preferred</u>
Virtual	-0.01 (0.02)	0.00 (0.01)	-0.14*** (0.02)	-0.18*** (0.01)
Lagged Math		0.20*** (0.01)		0.50*** (0.01)
Lagged ELA		0.59*** (0.01)		0.28*** (0.01)
Mid-year Switches		-0.06*** (0.01)		-0.06*** (0.01)
Black		-0.07*** (0.02)		-0.10*** (0.02)
Hispanic		0.03 (0.03)		-0.01 (0.03)
Asian		0.12** (0.06)		0.18** (0.08)
ESL		-0.11** (0.05)		-0.01 (0.04)
Female		0.12*** (0.01)		-0.09*** (0.01)
Special Education		-0.29*** (0.03)		-0.13*** (0.03)
FRPL		-0.10*** (0.02)		-0.09*** (0.02)
n=	6,824	6,824	6,780	6,780

Notes: Heteroskedastic-robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Heterogeneous Effects

Heterogeneous effects of the virtual school are presented in Table 5. Negative results in math are consistent across gender, race, FRPL status, and school level. In ELA, however, we estimate statistically significant heterogeneous effects for females relative to males and elementary schoolers (grades 3-5) relative to middle schoolers (grades 6-8). The estimate of virtual

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schooling on ELA for females is 5 percent of a standard deviation more negative than for males. We speculate these heterogeneous effects could be attributable to females being, on average, more social than males and less naturally inclined to thrive in the independent environment of online education. The estimate of virtual charter schooling on ELA for elementary schoolers is 7 percent of a standard deviation more negative than for middle schoolers. We speculate that middle school students are more comfortable using computers and maintaining concentration for long periods than elementary school students.

Table 5: Heterogeneous Effects

	<u>ELA</u>	<u>Math</u>
Female*Virtual	-0.05** (0.03)	-0.03 (0.03)
White*Virtual	-0.05 (0.03)	0.00 (0.03)
FRPL*Virtual	-0.00 (0.03)	0.02 (0.03)
Elementary*Virtual	-0.07** (0.03)	-0.03 (0.03)

Notes: Heteroskedastic-robust standard errors in parenthesis. Control variables include lagged test scores, mobility, race, gender, ESL, FRPL, and special education status.

*** p<0.01, ** p<0.05, * p<0.1

Limiting Sample to Non-Switchers

In the previous analysis, treated students are matched to comparison students who had the same number of school transfers, and no virtual enrollment, during the outcome year. In both the ELA and math samples, roughly 48 percent of virtual students received instruction in both the virtual school and another public school during the outcome year, which could obscure the treatment effect estimated in the previous section. To address this concern, we conduct a separate analysis

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restricted to the 52 percent of students who do not change schools during the outcome year.

These estimates are presented in Table 6.

Table 6: Virtual School Outcomes, Students without Switches in Outcome Year

	<u>ELA</u>		<u>Math</u>	
	Simple	Preferred	Simple	Preferred
Virtual	-0.04 (0.03)	0.00 (0.02)	-0.16*** (0.03)	-0.21*** (0.02)
Lagged Math		0.22*** (0.01)		0.52*** (0.02)
Lagged ELA		0.58*** (0.01)		0.29*** (0.02)
Black		-0.06** (0.03)		-0.09*** (0.03)
Hispanic		0.06 (0.04)		-0.03 (0.04)
Asian		0.17*** (0.06)		0.18** (0.09)
ESL		-0.19*** (0.06)		-0.06 (0.06)
Female		0.12*** (0.02)		-0.10*** (0.02)
Special Education		-0.28*** (0.03)		-0.12*** (0.03)
FRPL		-0.10*** (0.02)		-0.09*** (0.02)
n=	3,534	3,534	3,506	3,506

Notes: See appendix for student counts by grade and year. Heteroskedastic-robust standard errors in parenthesis. Limiting the sample to students without switches in the outcome year produces differences in observable characteristics much like those presented in Tables 3A and 3B, with one exception: differences in lag test scores in the non-dependent variable subject are exacerbated. In the ELA outcome sample, virtual students are more negatively selected with respect to prior year Math test scores. In the Math outcome sample, virtual students are more positively selected with respect to prior year ELA scores.

*** p<0.01, ** p<0.05, * p<0.1

Results that exclude switchers are fairly consistent with the findings from our main specification. In ELA, results remain nearly zero. In math, the estimated negative effect of virtual

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schooling is 21 percent of a standard deviation, which is 3 percentage points of a standard deviation more negative in magnitude than the math estimate in Table 4.

One-Year Outcomes, Without Mobility Controls

The final analysis of one-year outcomes uses the same matching protocol to generate a comparison group for the virtual students, with one exception: we exclude mobility in the matching process and as a control variable. Tables 7A and 7B present observable differences, after matching, between virtual students and their peers.¹⁴ This protocol creates groups of students who are similar in most characteristics but who might differ on mobility during the outcome year. Statistically significant differences persist on lagged test scores in the non-dependent variable subject, FRPL status, and ESL status. The groups are indistinguishable with respect to prior test scores in the subject of the dependent variable, gender, special education status, and race.

¹⁴ These sample sizes are marginally larger than those presented in Tables 3A and 3B because excluding mobility from the matching protocol allows fewer virtual students to be dropped from the analysis. Fewer than one percent of virtual students are dropped from both ELA and Math outcome samples, respectively.

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Table 7A: Observable Differences After Matching, Without Mobility (ELA Outcome)

	Virtual	Comparison	Difference	
Lagged ELA (normed)	0.08	0.08	0.00	
Lagged Math (normed)	-0.08	0.11	-0.19	***
Female	0.53	0.53	0.00	
FRPL	0.58	0.52	0.06	***
ESL	0.01	0.04	-0.03	***
Special Education	0.08	0.08	0.00	
White	0.73	0.73	0.00	
Black	0.15	0.15	0.00	
Hispanic	0.07	0.07	0.00	
Asian	0.01	0.01	0.00	
	n=3,467	n=3,467		

Notes: See appendix for student counts by grade and year.

*** p < 0.01, ** p < 0.05, * p < 0.1

Table 7B: Observable Differences After Matching, Without Mobility (Math Outcome)

	Virtual	Comparison	Difference	
Lagged ELA (normed)	0.07	-0.02	0.09	***
Lagged Math (normed)	-0.08	-0.08	0.00	
Female	0.53	0.53	0.00	
FRPL	0.58	0.55	0.04	***
ESL	0.01	0.04	-0.03	***
Special Education	0.09	0.09	0.00	
White	0.73	0.73	0.00	
Black	0.15	0.15	0.00	
Hispanic	0.07	0.07	0.00	
Asian	0.01	0.01	0.00	
	n=3,473	n=3,473		

Notes: See appendix for student counts by grade and year.

*** p < 0.01, ** p < 0.05, * p < 0.1

Table 8 displays achievement differences among students who are not matched on mobility. Under this specification, virtual students perform 4 percent of a standard deviation worse in ELA and 21 percent of a standard deviation worse in math than their matched peers. Our primary research interest is the difference in these estimates relative to those obtained using our mobility-matched sample.

Table 8: Virtual School, One-Year Outcomes without Mobility Controls

	<u>ELA</u>		<u>Math</u>	
	Simple	Preferred	Simple	Preferred
Virtual	-0.08*** (0.02)	-0.04*** (0.01)	-0.19*** (0.02)	-0.21*** (0.01)
Lagged Math (normed)		0.21*** (0.01)		0.51*** (0.01)
Lagged ELA (normed)		0.61*** (0.01)		0.27*** (0.01)
Black		-0.05*** (0.02)		-0.11*** (0.02)
Hispanic		0.04 (0.03)		-0.02 (0.03)
Asian		0.10* (0.06)		0.19*** (0.07)
Mix		0.01 (0.03)		-0.05 (0.04)
ESL		-0.18*** (0.04)		-0.06 (0.04)
Female		0.12*** (0.01)		-0.08*** (0.01)
Special Education		-0.25*** (0.03)		-0.15*** (0.03)
FRPL		-0.11*** (0.01)		-0.11*** (0.01)
N=	6,934	6,934	6,946	6,946

Notes: See appendix for student counts by grade and year. Heteroskedastic-robust standard errors in parenthesis.
 *** p<0.01, ** p<0.05, * p<0.1

The findings in Table 8, which do not account for mobility, are 3 percentage points of a standard deviation lower in ELA and 4 percentage points of a standard deviation lower in math relative to the findings in Table 4, which do account for mobility. The ELA estimate in Table 8 is statistically significantly different than the ELA estimate in Table 4 at the 99 percent confidence level. The math estimate in Table 8 is statistically significantly different than the math estimate

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in Table 4 at the 95 percent confidence level. Thus, failing to account for mobility attenuates the point estimate of virtual charter schooling.¹⁵

While the bias is downward, as expected, it is smaller in magnitude than we hypothesized. Still, these results may demonstrate the importance of controlling for differential rates of school mobility. Had we not accounted for this difference in our primary analysis, we would have falsely concluded that the virtual school had a significantly negative effect on ELA outcomes when, we argue, the likely true effect is null.

Three-Year Outcomes

If differential mobility rates between virtual students and their peers downwardly bias estimates of achievement effects, virtual students enrolled in the same school for all four years may demonstrate stronger achievement relative to the one-year estimates including virtual students with higher mobility rates. To test this hypothesis—and to explore medium-term outcomes of virtual schooling—we restrict our treatment group to students enrolled in the virtual school of interest for all four years. This limits the treatment group considerably (n=244) because virtual students are inherently mobile and because students in this anonymous state are only tested in grades 3 through 8. To be included in the comparison group for this analysis, a student must have outcome data for all four years, never enroll in the virtual school, and have similar low-rates of mobility relative to the virtual students with whom they are matched.

¹⁵ The point estimates in Table 4 and Table 8 are obtained from different samples. We conduct significance tests between the estimates by merging both samples, eliminating duplicate student observations, and running our preferred specification with separate indicator variables for comparison students in each of the two samples. The regression coefficients for each comparison group indicator have a variance and a covariance because they are estimated in the same equation, allowing us to test for statistical significance between the two estimates.

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Table 9 presents descriptive statistics among students in the three-year outcome sample. Differences between treated and potential comparison students at baseline are similar to the differences measured in the one-year outcomes (Table 2). Lagged ELA test scores are higher for virtual students than their peers by 10 percent of a standard deviation. Lagged math test scores are lower for virtual students than their peers by 36 percent of a standard deviation. Virtual students are less likely to be identified as FRPL and ESL, 22 percentage points more likely to be white, and 20 percentage points less likely to be Black relative to their peers.

Table 9: Baseline Descriptive Statistics (2014-15), Three-Year Outcome Sample

	Virtual	Comparison	Difference	
Baseline ELA (normed)	0.09	-0.01	0.10	***
Baseline Math (normed)	-0.37	-0.01	-0.36	***
Mid-year School Switches	0.15	0.06	0.09	***
Female	0.45	0.49	-0.04	***
FRPL	0.60	0.67	-0.07	**
ESL	0.00	0.08	-0.07	***
Special Education	0.14	0.14	0.00	
White	0.73	0.51	0.22	***
Black	0.14	0.34	-0.20	***
Hispanic	0.03	0.09	-0.06	***
Asian	0.02	0.02	0.00	
	n= 244	n= 158,898		

Notes: *** p < 0.01, ** p < 0.05, * p < 0.1

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After matching, 36 out of the 244 students who enroll in the virtual school for all four years could not be matched to a comparison student with similar race, mobility, gender, and baseline scores in both academic subjects. Thus, the final treatment group sample is 208 students. Each treated student is matched to one comparison student in other public schools, resulting in an analytic sample of 416. During the baseline outcome year, 2014-15, there were 106 students in grade 3, 136 students in grade 4, and 174 students in grade 5.

Table 10 demonstrates that the treated and comparison group samples are nearly identical on observable traits after the match.¹⁶ The only statistically significant difference between groups is that virtual students are two percentage points less likely to be identified with ESL status. In our preferred regression specification, we control for ESL status and all other covariates.

¹⁶ Here, the same comparison group is generated for ELA and Math outcomes because our matching protocol achieves balance on prior year test scores in both subjects.

Table 10: Observable Differences After Matching, Three-Year Outcome Sample

	Virtual	Comparison	Difference
Baseline ELA (normed)	0.10	0.10	0.00
Baseline Math (normed)	-0.31	-0.31	0.00
Mid-year School Switches	0.06	0.06	0.00
Female	0.46	0.46	0.00
FRPL	0.58	0.62	-0.04
ESL	0.00	0.02	-0.02 **
Special Education	0.01	0.02	-0.01
White	0.77	0.77	0.00
Black	0.18	0.18	0.00
Hispanic	0.03	0.03	0.00
Asian	0.02	0.02	0.00
	n= 208	n= 208	

Notes: Mid-year school switches slightly exceed 0 for virtual students because some students observed in the virtual school for four years are also observed in non-virtual schools for a portion of a school year. These students are matched to peers with similar low rates of mobility.

*** p < 0.01, ** p < 0.05, * p < 0.1

Table 11 compares ELA and math achievement of virtual students observed in their same school for four years against a similar comparison group. For ELA, the estimated effect of virtual charter schooling in year one and year two is positive but not statistically significant. By year three, the estimate increases to 22 percent of a standard deviation and becomes statistically significant. In math, the estimated effect of virtual schooling is negative in years one and two, and it is statistically significant in year two (16 percent of a standard deviation). By year three,

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the negative estimate is not statistically distinguishable from zero. Figures 1 and 2 display these trends graphically.

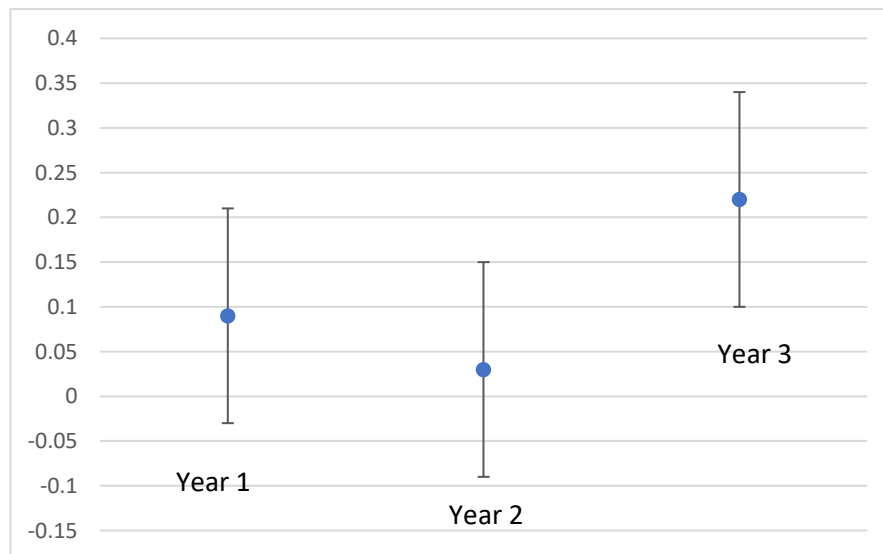
Table 11: Virtual School Three-Year Outcomes, ELA and Math

	Year 1	Year 2	Year 3
Virtual ELA	0.09 (0.06)	0.03 (0.06)	0.22*** (0.06)
Virtual Math	-0.09 (0.06)	-0.16** (0.08)	-0.10 (0.07)
n=	405	409	387

Notes: Heteroskedastic-robust standard errors in parenthesis. Control variables include baseline test scores, mobility, race, gender, ESL, FRPL, and special education status. The sample size changes during each year due to missing outcome data. Virtual students are 6 percentage points more likely to have missing outcome data, but the differential attrition is below the “cautious bound” recommended by the What Works Clearinghouse. We generate non-response weights and apply them to each year’s sample.

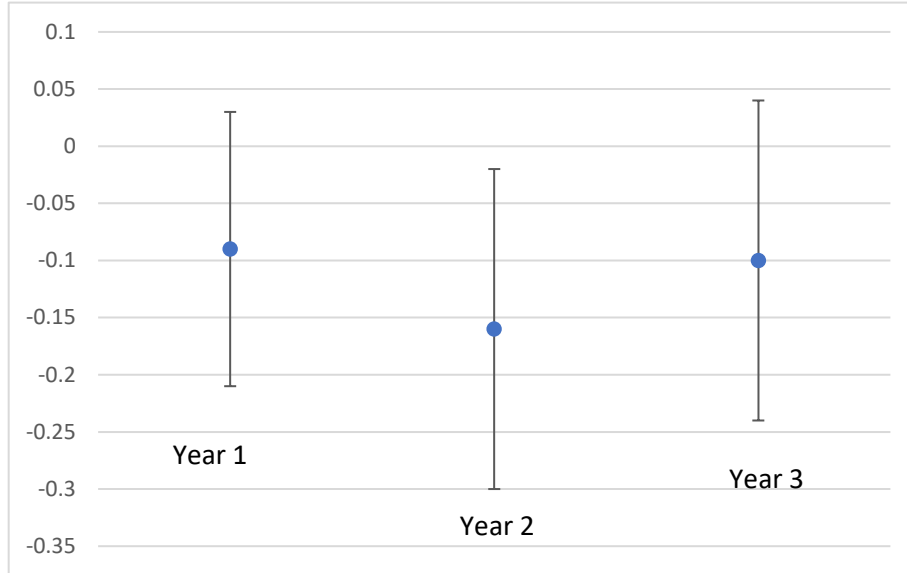
*** p<0.01, ** p<0.05, * p<0.1

Figure 1: ELA Achievement, Three-Year Outcomes



Notes: Each point represents the virtual school coefficient from Table 11, with 95 percent confidence intervals displayed.

Figure 2: Math Achievement, Three-Year Outcomes



Notes: Each point represents the virtual school coefficient from Table 11, with 95 percent confidence intervals displayed.

These estimates are noisier than the single-year outcome estimates and should be interpreted with caution due to the small sample size. By the third outcome year, students demonstrate greater achievement in ELA, and similar achievement in math, relative to matched peers. This may indicate that virtual schooling produces stronger outcomes when students remain enrolled for several years. In other words, there may be a certain type of student for whom virtual charter schooling is particularly effective. Another interpretation is that students who remain enrolled for several years are positively selected relative to the average virtual school student. Perhaps less mobile virtual students would be expected to produce greater achievement regardless of their school type. Still, students in this analysis are matched to peers with similarly-low rates of mobility, so any positive selection may approximately wash out on both sides of the comparison.

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The third outcome year in this sample occurs in grades six, seven, or eight. We reported earlier that middle schoolers may benefit more from virtual schooling than elementary schoolers, and thus it is possible that three-year outcomes for students who enrolled in virtual school beginning in kindergarten or first grade would not be as strong.

Limitations

We acknowledge this analysis relies on non-random selection of students into the virtual school of interest, although selection bias is mitigated by identifying comparison students with the same cohort, race, gender, and prior achievement as treated students. Although we cannot make causal claims about the impact of virtual charter schooling on achievement, we argue that controlling for student mobility in the outcome year represents an improvement on other quasi-experimental or observational studies of virtual schooling.

Absent random assignment, which is rare in virtual schooling, finding an appropriate comparison group for virtual students remains a challenge. Two otherwise identical students who only differ in virtual school enrollment are likely to have meaningful unobservable differences. Moreover, we do not know why students in our study change schools. A transfer due to a parent getting promoted to a new job is not the same as a child transferring because of bullying, and different types of mobility likely have different impacts on achievement.

Another limitation is not being able to observe when a student transfers in or out of a school during the year. Accordingly, in the one-year analysis, nearly half of the treatment group received instruction in both the virtual school and a brick-and-mortar school during the outcome year. The true effect of the treatment may therefore be “watered down.” We mitigate this obstacle by employing a sample restriction to students who do not change schools during the

outcome year and find similar results employing both the restricted and unrestricted one-year samples.

Conclusion

As of this writing, Covid-19 is disrupting brick-and-mortar education and increasing interest in the efficacy of virtual schooling. Our findings cannot be generalized to school years interrupted by a global pandemic, but we hope to shed light on the role of mobility in measuring achievement in virtual schools as well as the heterogeneous nature of virtual charter schooling.

We provide evidence that omitting controls for student mobility appears to downwardly bias the effects of virtual schooling on student achievement. Granted, part of the inherent promise of virtual schooling is the ability of such schools to capably transition students into the world of online education. Regardless of whether virtual schools are fulfilling this promise, however, measuring a highly-mobile virtual student against a less-mobile non-virtual student does not produce an apples-to-apples comparison. We suggest researchers account for school transfers when studying virtual schools because it is an observable characteristic influencing both virtual enrollment and achievement. Moreover, mobility likely proxies effectively for relevant unobservable student characteristics.

Although most existing research finds virtual schools have negative achievement effects, our analysis of a single, large, anonymous virtual charter school suggests that virtual schools are not monolithic. There may be considerable variation in how different virtual schools influence student growth. We estimate students in this virtual school demonstrate similar ELA growth in the short-term, and larger ELA growth in the medium-term, compared to peers in public schools. Estimated math effects, while still quite negative, are less negative in magnitude than found in other research.

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Finally, this analysis demonstrates that virtual schooling can produce achievement gains, for some students, relative to gains realized by similar students in traditional public schools.

Whether the students who benefit from virtual schooling are positively selected due to unobserved factors remains a question, but our results challenge the conventional wisdom that virtual schooling cannot increase learning.

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Appendix

The following tables identify the grade and outcome year for students included in each unique analytic sample. All grade by year cells include an equal number of virtual students and matched comparison peers in other public schools. Although students are only assessed in grades 3-8, and students must have a lagged test score to be included in the analysis, a third grade student can be included in the samples below if he or she is repeating third grade and has a baseline test score from the previous year.

ELA Mobility Cohort, Table 3A

	Grade 3	Grade 4	Grade 5	Grade 6	Grade 7	Grade 8	Total
2016	10	306	374	528	680	812	2,710
2017	22	342	226	350	416	470	1,826
2018	10	488	268	480	504	538	2,288
Total	42	1,136	868	1,358	1,600	1,820	6,824

Math Mobility Cohort, Table 3B

	Grade 3	Grade 4	Grade 5	Grade 6	Grade 7	Grade 8	Total
2016	10	310	372	534	678	796	2,700
2017	22	334	222	338	410	464	1,790
2018	12	492	268	460	506	552	2,290
Total	44	1,136	862	1,332	1,594	1,812	6,780

ELA No Transfers Restricted Cohort, Table 6

	Grade 3	Grade 4	Grade 5	Grade 6	Grade 7	Grade 8	Total
2016	6	218	262	366	456	586	1,894
2017	22	228	132	194	178	238	992
2018	4	250	62	82	102	148	648
Total	32	696	456	642	736	972	3,534

Math No Transfers Restricted Cohort, Table 6

	Grade 3	Grade 4	Grade 5	Grade 6	Grade 7	Grade 8	Total
2016	6	220	260	364	452	586	1,888
2017	20	226	130	190	172	232	970
2018	4	250	56	80	104	154	648
Total	30	696	446	634	728	972	3,506

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ELA No Mobility Cohort, Table 7A

	Grade 3	Grade 4	Grade 5	Grade 6	Grade 7	Grade 8	Total
2016	10	312	376	538	692	820	2,748
2017	24	342	230	352	420	476	1,844
2018	10	498	280	484	518	552	2,342
Total	44	1,152	886	1,374	1,630	1,848	6,934

Math No Mobility Cohort, Table 7B

	Grade 3	Grade 4	Grade 5	Grade 6	Grade 7	Grade 8	Total
2016	10	314	376	538	692	820	2,750
2017	22	342	228	352	428	484	1,856
2018	12	500	284	484	510	550	2,340
Total	44	1,156	888	1,374	1,630	1,854	6,946